DOCUMENT RESUME

ED 021 883 UD 001 960

By- Shulman, Lee S.

RECONSTRUCTION IN EDUCATIONAL RESEARCH.

Pub Date May 66

Note- 44p; Paper presented at Conf. on Social Change and the Role of Behavioral Scientists (Atlanta, Ga., May 4-6, 1966).

EDRS Price MF-$0.25 HC-$1.84

Descriptors- EDUCATIONAL ENVIRONMENT, *EDUCATIONAL RESEARCH, *ENVIRONMENTAL INFLUENCES, LANGUAGE DEVELOPMENT, *LEARNING PROCESSES, *RESEARCH NEEDS, STUDENT BEHAVIOR, STUDENT MOTIVATION, TRANSFER OF TRAINING

In this paper a description of educational processes serves as a model for a discussion of needed changes in the strategies and contents of educational research. The educational system in which a student functions is composed of his primary environment, the instructional environment, and the eventual transfer environments. When there is congruence among the elements of these environments, the educational process functions effectively. Breakdown in the process occurs when the elements are discontinuous. It is suggested that the most critical areas for educational research are studies of environmental differences, of the acquisition and use of language, and of the ways in which motivational patterns may be modified. Truly basic research, as distinguished from applied research, can be carried out within a framework of practicality and applicability. Variables which are amenable to change can be selected for study from the viewpoint of "knowledge for" instead of "knowledge of." Responses to this paper are included. (NH)

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
OFFICE OF EDUCATION

THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL OFFICE OF EDUCATION POSITION OR POLICY.

01960

Dr. Lee S. Shulman:

RECONSTRUCTION IN EDUCATIONAL RESEARCH

A common interest has brought all of us here today. It is a concern with the conditions under which we can most effectively change many forms of human behavior. In fact, it is because we view as human *behavior* what many others insist is human *nature* that we can discuss the topic of social change. Somewhat paradoxically, it is in the nature of man to change himself and others. This may be the essence of the difference between man and beast.

As we move up the phylogenetic scale, we find that the period of childhood characteristic of the different species increases in length. Moving from the less complex to the more complex organisms, the relative proportions of instinct to learning as forces influencing behavior rapidly change, until, with man, the role of learning is so central that the concept of instinct becomes almost absent. The major discontinuity between our species and all others lies in what McNeil has called this "systematic developmental retardation."

> Indeed, the helplessness of human young must at first have been an extraordinary hazard to survival. But this handicap had compensations, which in the long run, redounded in truly extraordinary fashion to the advantage of mankind. For it opened wide the gates to the possibility of cultural as against merely biological evolution...Biologically considered, the interesting mark of humanity was systematic developmental retardation, making the human child infantile in comparison to the normal protohuman. But developmental retardation, of course, meant prolonged plasticity, so that learning could be lengthened. Thereby, the range of cultural as against mere biological evolution widened enormously; and humanity launched itself upon a biologically as well as historically extraordinary career...By permitting, indeed compelling, men to instruct their children

UD 001 960
ED021883

Paper presented at the Conference on Social Change and the Role of Behavioral Scientists (Atlanta, Ga., May 4-6, 1966).

> in the arts of life, the prolonged period of infancy and childhood made it possible for human communities to eventually raise themselves above the animal level from which they began. (McNeil, 1961; pp. 20-21)

Thus, the absence of instinct, the absence of prefabricated behavior patterns which program the organism and have him "ready to go" at a very early stage in his development, provide man with his most human characteristic; his educability.

So marked is the dependence of human young upon their elders that in our species we have come to create social and political institutions whose major role is that of optimizing the conditions under which our young can traverse this dangerous period and most fruitfully enter into adulthood. Such an institution is the school. This plasticity acts as a two-edged sword, for with plasticity comes not only the potential for limitless growth, but also the danger of inestimable damage.

Recognizing that man's education and the scope of his educability are the most uniquely human things about him, it seems only appropriate that those disciplines which purport to study man and his nature should concentrate rather heavily upon studies of his schooling. American psychology at the beginning of this century did precisely that. The great men of that period of American psychology, William James, E. L. Thorndike, G. S. Hall, Robert Woodworth, John Dewey and others, were vitally interested in studies of the educational process. Such investigations lay at the hear of American psychological thinking during that time. However, beginning with Lloyd Morgan's Canon ruling unobservable mental processes out of bound for psychological study, the discipline of scientific psychology in America was slowly transformed from James' "Science of Mental Life" to Watson's "Science of

Behavior." And in that generally healthy antimentalistic revolution were discarded, not only the bath-water of Titchener's introspectionism but also, tragically, the baby of experimental educational research. Despite Thorndike's continuing admonitions that the proper laboratory for the psychologist was the classroom, and its proper subject, the pupil, the study of infrahuman species and their behavior dominated psychology. It would be an overstatement to assert that the study of school learning disappeared completely from the psychological map. Yet, it would be even more misleading to deny that such studies now occupied a rather peripheral role in the developing tradition of American experimental psychology.

The only area of educational psychological research which remained unscathed by this revolution was that of the then still infant investigations of mental measurement. This tradition, growing out of the work of Binet and Simon, Cattell, Terman and others, continued to flourish and received its greatest impetus from the success of mental testing during World War I. The emphasis of this movement was quantitative and descriptive. The objectives were the careful measurement of individual differences in human abilities. The schism between the respective *Weltanschaungen* of experimental psychology and mental measurement grew progressively wider, and it was not totally inappropriate that for many years educational psychology was identified with educational measurement, an observation whose consequences we will examine a bit later.* It is only in the most recent period that we have begun to see a reversal of these trends. The two traditions are once again beginning to coalesce. The major part of this paper

---

* For a full discussion of the meaning and implications of this distinction, cf. Cronbach (1957).

will be devoted to an examination of the most productive ways in which the aspects of each of these approaches to research that are most likely to yield fruitful consequences for education can be identified and acted upon.

A Model for Teaching and Transfer

It is somewhat disturbing to see how, in the history of education, so many promising ideas have become caricatures when carried both too far and too long. Probably the classic example of how a fine idea became caricatured when institutionalized was the progressive distortion of Dewey's original conceptions as they became institutionalized into Progressive Education. There is no need for me to go into a long description of the perversion of Dewey's ideas by progressive educators at this point. Suffice it to say that these kinds of distortions usually occur when we forget that the recommendation of changes must occur simultaneously at both an intellectual level and a rhetorical level. That is, while we recommend that practices change in a particular way, at the same time, since we want our audience to act on our ideas rather than just think about them, we must present these ideas in a manner likely to evoke the desired changes. In so doing, the principles of rhetoric demand of us that we overstate certain distinctions, set up as straw-men specific positions that we might otherwise view much less critically, and in general attempt to create an atmosphere which will most likely be receptive to our recommended changes. Too often, as was the case with Dewey, our listeners remember the arguments much better than the recommendations.

A more recent example may be seen in one of the most pervasive themes in education, which is repeated in all text books and courses in the field;

the insistence upon behavioral definitions of objectives for instruction. The emphasis of this position is upon "How do I want the observable behaviors of my students to change as a result of any given program of instruction?" On the whole, the effects of an insistence upon behavioral statements of objectives have been very beneficial to education. They have forced educators to think in great detail and with extreme precision about the things they do and the results they seek. They have tended to discourage the proliferation of platitudes and slogans as the basis for instructional and currical decisions. They have also forced educators to consider much more seriously the role of evaluation as an integral step in the instructional process.

In some ways, however, the emphasis upon the observable immediate behaviors of students has led to effects detrimental to clear thinking about education. It has often led to thinking about instructional objectives in terms of objective test items. We so often find ourselves asking not, "What do you think these students ought ultimately to be able to do?", but rather, "What kinds of items do you think these students ought to be able to pass at the end of the instructional sequence?" Thus, instead of allowing our major objectives to dictate the characteristics of the lessons presented and the assessments made, we have of late found ourselves in the position of allowing the limitations of our evaluation instruments and our measurement capabilities to delimit the scope of the objectives we raise. This is somewhat parallel to the too-often exaggerated claim that test makers are dictating the contents of the public school curriculum.

One of the critical things for teachers to recognize is that, except in a few situations, the ultimate objectives of instruction are not to

change immediately observable behavior, so much as they are to modify the *likelihood* that certain of these behaviors will occur in certain other situations. Thus, the mother who slaps the hands of her infant when he approaches a hot stove is not only concerned with his immediate present actions. She is also interested in lowering the likelihood that he will subsequently approach, not only that stove, but other dangerous objects. The public school teacher who organizes 'citizenship' activities in her classroom, such as student government with meetings, elections and the like is, hopefully, concerned that the behaviors learned in that setting effectively transfer to the students' eventual political behavior as adults. Similarly, the observation that a teacher-in-training can, on an objective end-of-term test, give an appropriate response is significant only if her corresponding behaviors in the classroom are equally appropriate.

In the schools, we presently judge whether a given act of learning has taken place by examining whether, at a subsequent point in time within *essentially the same setting*, a pupil is able to respond appropriately to a given pattern of stimuli. Even though the particular test item we use is not identical to the learning task on which a given piece of instruction was transacted, the fact remains that the paper-and-pencil evaluation takes place in the same classroom under time pressure and in competition for goodies with other members of a group.

Ultimately, however, our objectives are to modify the likelihood that a given change in behavior will carry over to some situation *outside* the classroom. I think a simple formulation here would be in order. This formulation is that the likelihood that a given change in behavior will carry over from situation X, in which it was taught, to some other situation Y, is a direct

function of the similarity, both perceived and actual, between the teaching situation (X) and the transfer setting (Y). To the extent that the teaching situation is congruent with the desired transfer situation, the likelihood will be high. To the extent that they are very dissimilar, the likelihood of transfer is quite low. At least two factors are operating here. The first is the extent to which the characteristics of the teaching situation that call forth the desired pattern of responses are present in the transfer situation. I may be perfectly capable of performing in a particular manner when I recognize that I am in a situation which calls for such performance. If, however, I am unable to recognize such a situation when it occurs because it is so unlike the setting in which I was taught, the fact that I have mastered that pattern of behaviors is irrelevant. I will not perform appropriately because I do not know that now is the time to do so.

A second factor interfering with appropriate performance in the transfer situation is the presence of competing responses. As the transfer situation becomes less and less similar to the original teaching situation to which we have been referring, the likelihood increases that it is becoming more similar to some other teaching situation, whether formal or informal, to which the student has been exposed. Growing out of these other teaching situations are *other* sets of responses which can conceivably compete with responses we have been teaching. For example, we understand that the lower-class child is generally taught to speak a very different language in the schools than he has learned at home. Which of these two kinds of language emerge in a given situation will be a function of the competition between these two total sets of responses. The winner of the competition is likely to be that language pattern which was learned in a setting most similar to

the specific transfer situation at hand.

We must recognize that, in addition to the instructional setting and those to which the products of instruction must eventually transfer, there is yet a third setting which must be taken into consideration. There is that group of settings from which the student comes to us. Prior to any contact with formal instruction and coincident with that instruction throughout his school years, every student is being trained, albeit informally, to function in a matrix of home, family and peers which we might call his "primary environment." In this setting, he develops skills and strategies of learning, attitudes about himself and others, predispositions to react to a range of external demands, and all manner of other things which are directly parallel to the kinds of things we expect to teach him in the schools. It may be fruitful to view the likelihood that a given student will perform successfully in the classroom setting as a function of the similarity between his primary environment and that of the classroom. That is, just as the likelihood of successful transfer of learning to some ultimate outside pay-off setting is a consequence of the classroom-to-outside similarity, so the likelihood of successful performance in the classroom setting itself is a function of the similarity between that setting and the setting from which the student is coming, his primary environment.

Too often we view the students coming into our classrooms as examples of the classical *tabula rasa*, the blank slate of epistemology, upon which anything we write with the stylus of instruction cuts deeply and without interference. At best, we are willing to view the variability among students only in terms of individual differences in supposedly immutable characteristics, as exemplified by the usual interpretations of such

measures as I.Q. Thus, students are seen to differ in the speed with which this instructional stylus can write upon the wax of their minds and the amount of pressure we must apply to make a firm impression. Since such mental characteristics are 'obviously' not subject to instructional modification, we set up ability groups to which we assign these students and from which they are never likely to become paroled. That such groupings also, by some accident, happen to reflect the social and economic status differences among our students is considered of little interest. In such a manner does the school succeed in preserving the social and cultural *status quo*.

We thus see that success in coping with the demands of the school setting is a function of the similarity between primary and school environments. For some children, the school setting is simply a continuation of the kinds of demands, expectations and rewards which characterize their home environments. For others, the school and the primary environments are literally two different worlds with little chance of fruitful transfer.

In summary, we observe that adequately to examine the nature of instruction requires that we attend closely to more than the activities of teachers and students in the classroom, for this is but one of the three critical settings through which students are moving in the educational process. Instead, we must view education in terms of the complex interrelationships among the primary, the instructional and the transfer settings. The important questions of education are questions of how we can facilitate the movement of students among these environments.

The Study of Environments*

We are, as social scientists, dramatically impotent in our abilities to characterize environments. Generally, we do not even try. It should, by now, be a truism to point out that neither individuals nor groups can be adequately described without reference to some setting. Thus, for Dewey the starting point of his discussions, be they of education or aesthetics, was always 'some organism in some environment.' Henry Murray (1938) posited two equally important categories for his studies of personality: *needs* and *press*, i.e., person variables and environment variables. The language of the behavioral sciences is in great need of a set of terms for describing environments that is as articulated, specific and functional as those we already possess for characterizing individuals.

An example that is close to all of us is the continued use of such gross terms as "deprived" or "disadvantaged" to characterize the environments out of which come many of the children of the poor. Labelling the setting as "disadvantaged" does not, of course, tell one anything about the characteristics of that environment. Yet, we seem unable to progress beyond such simple dichotomies as "advantaged-disadvantaged." Reviewers and critics of research are finally beginning to realize that even those few categories which attempt to describe environments, such as *social class*, have been remarkably ineffectual in pinpointing the relevant differences in the backgrounds of individuals. (Karp and Sigel, 1965).

Imagine what the field of nutrition would look like if the nutritionist, in his attempts to characterize the nutritional status of the diets

---

* Much of the content of the ensuing section has been greatly influenced by the thinking of Benjamin S. Bloom (1964).

of individuals, were to be limited to a distinction between well-nourished and malnourished individuals. We would be quite skeptical of the value of generalizations such as "malnourished individuals have a higher incidence of respiratory ailments than well nourished," or "well-nourished subjects were observed to run significantly faster than malnourished subjects." Yet, are our pronouncements about all the differences between culturally-deprived and culturally-advantaged children any more fruitful? And are the myriadic studies contrasting lower-class and middle-class youngsters of any greater value? Such descriptive studies do not begin to suggest the necessary ingredients of experimental programs to change the conditions. Do we simply change all lower-class people to the middle-class?

The nutritionist can describe the nutritional environment of individuals in terms of caloric content, relative proportions of carbohydrates, fats and protein, the presence or absence of quantities of vitamins and minerals, etc. Possessing these kinds of precise terms allows him to plan ways of modifying the nutritional status of individuals in terms of highly complex, yet manageable patterns. Attaining such a level of facility in characterizing the *educationally-relevant* facets of environments should be one of the major goals of educational research. Without such an understanding, we are clearly handicapped in any attempt to make intelligent comparisons among proposed educational programs, (such as Headstart models) for these programs themselves are no more than carefully planned environments.

A number of behavioral scientists have begun to study the characteristics of environments in a systematic fashion. Among these, the work of Bloom (1964) is of special interest. Bloom reports many instances of great

improvement in the effectiveness of academic predictions when measures of the intervening environments are taken into account in the prediction equations. He emphasizes that we must replace our older, static terms for describing environments (e.g., social class) with dynamic, process variables (e.g., achievement press). As evidence for this assertion, he cites the research of Dave (1963) and Wolf (1964). The goal of all such predictive research should be, Bloom maintains, not the inexorable stamping of unavoidable fates on helpless children, but the identification of the critical processes contributing to those fates. Given an understanding of the process variables most responsible for the ultimate status of individuals in some growth area, we can begin to develop effective methods of modifying those processes and, hence, destroying the accuracy of our own predictions.

The work of Roger Barker and his colleagues (1965) reflects a totally different set of strategies for studying the environment; those of ecological psychology. Pace and Stern (1958) have applied the tools of psychological measurement to the task of characterizing the essential differences between college environments. Jules Henry (1963) has used the methods of anthropological investigations to study the home and school as elements of culture. From his work has come the compelling concept of the "hidden curriculum" in the middle-class home. It is only through such environment-centered research that behavioral scientists can develop adequate terms to describe the relevant attributes of the three educational environments which were discussed earlier; the primary, instructional and transfer settings.

From the still sparse literature on the relationships between environmental variables and behavior, I shall now discuss two areas that seem most crucial; the development of language and the development of motivational patterns.

Language

It is quite clear that one of the major ways in which those children whom we label 'disadvantaged' differ from other children is in their inability to use language effectively. Whereas for the middle-class child language is a flexible tool which broadens the range of activities he can perform effectively, for the lower-class child language more often acts as a set of limiting conditions, constricting his activities rather than opening them up. Jensen and Rohwer (1963) have reported that the superiority of a group of culturally advantaged youngsters in a paired-associates task is probably due to the availability to them of verbal mediators which are unavailable to disadvantaged children. Martin (1965) has demonstrated that mentally retarded subjects can perform as well on a paired-associates task as subjects of normal intelligence if they are supplied the verbal mediators necessary for effective performance. When these mediators, which Martin calls "associative strategies," are supplied to the retarded subjects, the learning curves for the groups coalesce. Hence, the language deficits manifested by children who learn less effectively can probably be reversed by proper training. But what is likely to be the reason for these language deficits?

Loban (1965) presents developmental evidence of the greater language proficiency of middle-class children. Milner (1951) and Strodtbeck (1965) suggest that, in addition to the models of language which the parents of these children present in the home, a major cause of language deficits among lower-class children is the general pattern of parent-child relations. Strodtbeck describes the absence of conditions in the lower-class home in which the child can use language as a bargaining tool. Language, in

these situations, is viewed as an intrusion rather than a tool. Thus, two aspects of environment will require intensive study if we are better to understand the language development of disadvantaged children: the nature of the language models in the primary environment to whom the children are exposed and respond, and the kinds of interactions among the family members in which language plays a part.

Motivation

Discussions of motivation usually revolve around problems of action. Parents and teachers express concern that pupils should want to 'do something' rather than just 'sit there.' Ironically, the major motivational problem of the poor speaker and learner may be the very opposite. He must be taught, not to act, but to withold action. Dewey (1938) described the first stage of the process of inquiry as the delay of action. One must stop before he can think. Recent studies by Kagan, *et al* (1964) have contrasted impulsive children, who delay but a short time before attempting to solve problems of high stimulus-uncertainty, with reflective children, who wait much longer before making their first response. The observed styles are extremely stable and the reflectives make far fewer errors. Kagan (1966) has reported an additional study which suggests that lower-class subjects are far more impulsive than middle-class. This is confirmed by many other reported findings of less ability to delay immediate gratification on the part of lower-class individuals.

There is a close relation, I believe, between the language problems discussed earlier and the problem of delay. Those who study the relationships between learning and language usually speak of the availability to the learner of the appropriate 'mediators.' To 'mediate' is to be in the

middle of something. It is also the opposite of 'immediate.' Hence, the problem of finding the appropriate language form to put into the middle of something that must be learned is ultimately double-barrelled. One must be capable of delay, which is the suppression of the impulse to respond *im*-mediately, and one must possess the language tools with which the ensuing delay can be utilized most productively.

The capacity to delay and the capacity to use language effectively are bound up together as underpinnings of human intelligence and academic achievement. Our research responsibility is to discover those environmental process variables that account for individual differences in those capacities and to use that knowledge to modify the development of young children.

Reconstruction in Research Strategy

In order to cope with these and other educationally relevant problems, behavioral scientists will have to dispense with many of the comfortable approaches to science which have heretofore characterized their scholarly endeavers. The greatest part of education involves learning by children in groups. I have no doubt that the classical theories with which we ply our teachers-in-training and graduate students are quite poverty-stricken in their abilities to illuminate such educational processes. The relationship we discussed earlier between the situation in which something is taught and the likelihood of its effective transfer to some other situation holds here as well. To the extent that research is conducted in a setting similar in its characteristics to the school situation, to that extent you will get reasonable extrapolations and applications from it to the classroom milieu.

Given this information, it should be no surprise that the history of behavioral science research in education is not particularly glorious. The many differences between the human learning laboratory and the typical classroom are numerous. Needless to say, the differences between the animal learning laboratory and the classroom are far greater. Yet, we have been all too quick to generalize from even the latter setting to classroom behavior. In discussing the inadequacy of psychoanalysis as a general personality theory, Bruno Bettelheim (1960) cites quite parallel conditions. He points out that psychoanalysis was doomed to failure as a general personality theory because all of its generalizations were extrapolations from that most restricted of experimental settings, the psychoanalytic couch. In the same manner, does it not seem presumptuous to expect that a learning theory based upon evidence from the T-maze, the pigeon's press-bar or the memory drum can effectively be used to direct the planning of that most complex of human endeavors, the typical classroom?

The questions of education are ultimately experimental. We ask what the optimal conditions are for evoking a desired set of behavior changes in children. Yet, it is quite striking that whereas our major educational problems are clearly experimental, the dominant psychological tradition in education has been descriptive and correlational. The one consistent theme in educational research for the past 65 years has been psychometric. From Binet to Project Talent, the emphasis has been upon the measurement and prediction of individual differences among children. And, though usually not stated explicitly as such, a feeling about the immutability of these characteristics accompanies this tradition. Even now, as we examine the literature on education for socially disadvantaged children, we find that

the majority of the studies have been attempts to identify and describe the differences between advantaged and disadvantaged subjects. Gordon (1965) in his review of literature on education for the socially disadvantaged, finds it necessary to point out that the observation that a set of variables covary under a particular set of circumstances does not at all demonstrate that by modifying one we will necessarily cause a change in the other. Thus, these correlational studies give us not one bit of evidence about causation and hence no real help in planning strategies of interference for modifying the developmental paths of the children who are being studied. It is when such "experimental" answers are extrapolated from correlationally-studied questions that we contribute to the growing mythology which controls so much of our behavior as educators.

Examples of such myths surround us in poignant abundance. We are told that middle-class teachers are incapable of successfully teaching lower-class children. Yet, not only is there no experimental evidence to confirm that assertion, there is considerable evidence that becoming a teacher renders any ostensibly lower-class individual a member of the middle class. Another kind of myth pervades the reading area. We are told by countless teachers that no child can learn to read if he has not reached a mental age of six and one-half. Needless to say, here again the absence of experimental evidence is dramatic. Contemporary education has created its own massive Olympus upon which the multitude of pedagogical gods, equally devoid of empirical bases, constantly do battle. Our problem is to discover approaches to research in education that will remedy this shameful state.

Research in education wil have to turn its back upon the safe and sterile surroundings of the laboratory and address itself to that most threaten-

ing of settings for the educational researcher, the classroom. Instead of viewing experimental treatments in terms of single variables, such as "nursery-school" versus "no nursery-school" or "discovery" versus "rote," we must begin to contrast total educational approaches, e.g. curricula or their parts. In so doing, however, we cannot simply bootleg in methodologies from other disciplines on the premise that they will work for our problems as well as they have worked for others. The story is told of the man who came upon Calvary on that fateful day, and stopped for a few moments to witness the crucifixion. As he turned to leave, he happened to spy, on a distant hill, three more crosses with men being crucified upon each. He turned to another man who appeared to be some kind of official and asked, "I have heard about what is taking place here at Calvary, but can you tell me what is happening out there on that other hill?" The official turned to him and replied, "Of course. That's our control group!"

For our present purposes, the concept of a set of experimental groups which are equivalent to each other in all matters save one dimension whose effect is being studied, all of which are in turn compared to some 'control group' is probably an anachronism. Cronbach (in press) has pointed out that on those rare occasions when an expensive and slowly developed new curriculum is subjected to intensive evaluation, the pattern usually looks something like this. The new materials, which are the results of a team of scholars and educators spending millions of dollars over a period of years, are contrasted with what Mr. Jones, the chemistry teacher, has been doing for the last decade. To no one's surprise, the new materials come out smelling like a rose. Yet, no one considers the possibility that an equivalent amount of time expended upon improving whatever it is that Mr. Jones has been doing

for ten years might yield equally dramatic results for his methods. We must forget the conception of clean control groups and replace it with the notion of comparing, under reasonably controlled conditions, the best examples of educational conditions we can find which, in fact, are alternatives for us. In Cronbach's words, if you wish to compare a camel with a horse, find the best example you can of each and compare them. You don't get two camels and cut the hump off of one.

Because of the fact that educational programs are far more complex than the present psychological theories which purport to explain the teaching-learning process, it might be in the long range interest of both psychological theory and education to ignore those theories for the moment and proceed along an atheoretical path in the study of education. If we but look around, we will see that in contrast to our theoretical impotence, we do not lack for ideas and even numerous successes in the teaching of many things to a wide variety of children. In fact, were there not a fairly large proportion of successful teaching experiences with working class children, a strikingly large proportion of those of us present today would not be here. What we must do is construct new educational programs around the principles which we *think* are operating in presently successful educational programs, and then enter into the schools and begin engineering like mad. We must compare and contrast, not tightly controlled treatments whose differences are based upon psychological theories, but rather total programs, each of which is seen as the best example we can now present of some particular approach. We may find the myriad of existing descriptive studies useful in this endeavor, but probably only as general guides.

What ought to be the new characteristics of this new breed of research strategies with which I would replace our presently nonexistent or shopworn collection? Just as I previously delineated a few of the substantive research areas most likely to yield results relevant to effective social planning, I will now attempt to describe a few of the general research strategies which I feel have the greatest promise for effective educational research.

The three such strategies which I would like to discuss are (1) the experimental-longitudinal strategy, (2) the epidemiological strategy and (3) the engineering strategy.

Experimental-longitudinal designs are studies in which we examine the long-term effects of continued educational programs. As with experiments in general, we attempt to equate the groups of subjects at the beginning of the study. That is, either through randomization or careful selection, we identify equivalent groups of children for each of the treatment programs. Unlike the typical experiment, however, we are looking at the cumulative effects of ongoing programs rather than the one-shot effects of a single exposure. Hence, the educational program is something that continues over a period of months or years. The evaluation of these programs likewise is continuous. The criterion variables would be broadly stated to cover as wide a possible range of relevant behaviors on the parts of the children as possible. All attempts would be made to insure that the experimental purity of the groups would be maintained, just as in any experiment.

Epidemiological strategies grow out of the kind of research often conducted in the public health field. Often an epidemic of some disease will spread through an area very rapidly, affecting some parts of the population

and leaving others unscathed. An important question raised here is what distinguished those who were susceptible to the disease from those who emerged unharmed. Likewise, in studies of such social phenomena as delinquency, it appears that some people may come from a given environment and turn to crime while others, ostensibly from the same setting, turn to more socially acceptable activities.

I believe we are warranted in inferring that while, in either case, the two groups in question may appear to come from the same setting, there are some significant differences betweeen the two. Although we did not create these two groups experimentally, we can ask and find answers to a number of highly critical questions by identifying representative members of the two groups and attempting to analyze back and discover all of the differences between them. From such an analysis, hopefully will come working hypotheses about the kinds of purposeful differences in treatments we might develop either to raise the probability of immunity or the development of socially acceptable behavior patterns on the parts of future individuals. These hypotheses could, in turn, be tested experimentally in experimental-longitudinal designs.

Thus, instead of becoming satisfied with distinctions between lower and middle class, or advantaged and disadvantaged children, we should recognize the fact that whenever differences between such groups are discovered there is usually a high degree of overlap between the samples. For example, there are many lower-class children that do far better on intelligence tests than many middle-class children. In the epidemiological strategy, we would differentiate those lower class children who do poorly from those who do well and attempt to identify, in the much more precise environmental process terms

discussed earlier, the differences between these parts of our general lower-class group. We will probably find that those differences we discover are far more functional for the planning of educational interference programs than the kind usually generated by our standard lower-class/middle-class comparisons.

Engineering strategies are also the "try it and then analyze back" variety. One way of doing this is to go out into the field and identify teachers or programs that appear to give some consensual feeling that they are being successful with a given group of children for whom we have no systematic theories of instructional effectiveness. By careful analysis of what the teachers who appear to succeed with some groups do in contrast with those who do not, we might identify some principles which could subsequently be tested with experimental-longitudinal designs. Another variation on the engineering strategy is simply to go out and try, without any particular recourse to prior theorizing, a whole range of instructional approaches until we find a few that seem to be particularly effective. Having then identified those that are effective, we can subsequently reflect about them and analyze them to try to discover the principles which distinguished them from the ineffective methods.

The time has also arrived for educational researchers to divest themselves of the yoke of statistical hypothesis testing. Rather than set arbitrary levels of statistical significance against which our findings are tested, we must be prepared to identify the range of differences observed in an experiment that will have pedagogical or developmental significance. The appropriate research strategy here is not statistical significance testing wherein we demonstrate that our findings are unlikely to have occurred through

sheer good fortune, but rather the strategy of *replication*, wherein we demonstrate that we can reproduce our results, whatever their magnitude, whenever we feel so inclined.

The kinds of strategies I have just described are not noteworthy for their elegance or symmetry, nor for their parallel to the great and time-honored traditions of the physical sciences. Yet, the time has clearly come when we must stop using methodological preconceptions as the starting points for our research strategies and begin allowing the *problems* for which we seek solutions to dictate the most reasonable strategies for finding answers. If we wish to modify what happens in classrooms, we must study classrooms. If we are concerned with the cumulative effects of long-term programs, we must study programs over the long haul. Similar reappraisals of many of the major issues in psychology and education are currently taking place. (Shulman and Keislar, in press).

Studies of this kind are in vivid contrast to the type most often reinforced in academic circles. These latter studies are usually short, quick, and speedily analyzed. They are tightly designed with all variables well controlled. The experimental treatments can be administered in a matter of minutes or hours and the results are assessed immediately thereafter. They are as unlike the form of experiment we have been discussing as they are unlike anything that happens to children in real classrooms. Yet, on the grounds that the greater complexity of classroom activities constitute no more than a series of these more simple operations sequentially linked together to form a curriculum, the argument is made that such strategies of research are justified for educational investigations.

We must recognize, however, that there is also a network of institutions which work to reinforce this approach to research. As long as the academic setting is one where the patterns of reinforcement are contingent upon number of articles published by an investigator, rather than the relevance and quality of his investigations; as long as the overwhelming proportion of studies conducted in this country are one-shot doctoral dissertations designed to collect the most data in the shortest time, we will continue to encourage the type of research I have described. What is needed is more than a change in the way in which we train the next generation of researchers. We need a revolution in the kinds of criteria utilized by administrators who judge the quality of academic performance and parcel out the subsequent monetary and status rewards.

I can well imagine the following discussion between two pigeons in Skinner's laboratory:

Pigeon A: Say, how are you coming along on that novel you've been working on?

Pigeon B: I'm ashamed to admit it, but I've been so busy playing ping-pong lately, I haven't had a moment to write.

Pigeon A: Why, that's terrible. You told me that your novel was the most significant thing you would ever do.

Pigeon B: I know, and I still feel that way. But look, a bird's got to eat.

Note that this is not another repetition of the frequent complaints against "publish or perish" traditions in our universities. It is about time those who are active in research pointed out that the university professor's moral responsibility to further the growth of his discipline is at least as great as his responsibility to teach his students. In an era where social change stands out as one of the major goals of our society, the men who possess the skills necessary to identify the most effective conditions

for change must be encouraged to apply all their energies to such investigations. Research is not an indecent luxury enjoyed by the idle intellectual who wishes to be spared the burden of teaching undergraduates.

David Hawkins (in press) has suggested that the likely value of a piece of research is a direct function of the amount of preparation needed before it can begin. This is very true of the needed research in education. The designs we need are both experimental and longitudinal. We must look at the effects of long-term programs as they affect groups of students over relatively prolonged periods. Extrapolations from short-term studies to long-term programs are based upon additive notions about the effects of experience that are tenuous, at best. Yet, in order to reinforce the conduct of such investigations, the men who are in charge of passing out the reinforcements will have to modify their criteria.

Final Remarks

In this paper, I have attempted to discuss needed changes in the strategies and contents of educational research in terms of a model describing the processes of education. In this model, we suggested that the educational system in which the student functions is composed of three settings. These settings include his primary environment, the instructional environment and the eventual tranfer environments. To the extent that there is congruence among the elements of these settings, the educational process functions effectively. To the extent that the elements of these are discontinuous with each other, the process may break down at any one of a number of points.

Three specific research problem areas were examined because it was felt that these represented the most critical immediate areas for study in education.

These included careful studies of differences among environments, studies of the acquisition and use of language and studies of the modification of motivational patterns.

We then examined a number of research strategies which might yield more fruitful findings for educational application. It may be important at this point to indicate that I am not advocating here the conduct of only applied investigations. The purpose of research in the behavioral sciences is the acquisition of knowledge about human behavior. But knowledge can be of many kinds. Among these, a distinction can be made between "knowledge of" and "knowledge for." That is, it is possible to conduct truly basic research, yet select as the variables under investigation conditions that are amenable to change and hence can serve as input for educational programs and experimental study. I am suggesting that when an investigator embarks upon a study in which he will compare the performance of a number of groups in an area in which there are some important social ramifications to such differences, he selects for study variables that can be influenced. Instead of comparing in terms of white versus Negro or lower-class versus middle-class distinctions, can he not identify some meaningful process-terms and compare groups distributed along those dimensions?

This is still, I maintain, basic research. It need not suffer in some "status derby" because, though basic, it is also knowledge *for* some other purpose. In some circles, the very thought that the products of one's basic investigations might be trammeled by use in some practical setting traumatizes the researcher. Must we always fall back on allusions to Fleming-like serendipity as justifications of basic research? And must a basic researcher's planning of his investigations so that his findings might have relevance in

the "knowledge for" realm disqualify him as a *bona fide* basic investigator?

With Dewey, I feel strongly that the most basic questions of theory are also the most applied for students of human behavior. They all revolve around the question, "What are the optimal conditions for human growth?" He who asks and finds answers to that questions is making the most important contributions both to the behavioral sciences and to effective education.

REFERENCES

Barker, R. "Explorations in Ecological Psychology," *American Psychologist*, 1965, 20, 1.

Bettelheim, B. *The Informed Heart*. Glencoe: The Free Press, 1960.

Bloom, B. S. *Stability and Change in Human Characteristics*. New York: John Wiley and Sons, 1964.

Cronbach, L. J. "The Two Disciplines of Scientific Psychology." *American Psychologist*, 1957, 12, 11.

Cronbach, L. J. "The Logic of Experiments on Discovery." In Shulman, L. S. and Keislar, E. R. (Eds.) *Learning by Discovery: A Critical Appraisal*, Chicago: Rand-McNally, in press.

Dave, R. H. "The Identification and Measurement of Environmental Process Variables That Are Related to Educational Achievement." Unpublished Ph.D. Dissertation, University of Chicago, 1963.

Dewey, J. *Logic: The Theory of Inquiry*. New York: Henry Holt and Co., 1938.

Gordon, E. W. "Characteristics of Socially Disadvantaged Children." *Review of Educational Research*, 1965, 35, 5, 377-388.

Hawkins, D. "Learning the Unteachable." In Shulman, L. S. and Keislar, E. R. (Eds.) *Learning by Discovery: A Critical Appraisal*. Chicago: Rand-McNally, in press.

Henry, J. *Culture Against Man*. New York: Random House, 1963.

Jensen, A. and Rohwer, W. D. "Verbal Mediation in Paired-Associate and Serial Learning." *Journal of Verbal Learning and Verbal Behavior*, 1963, 1, 346-352.

Kagan, J., Rosman, Bernice L., Day, Deborah, Albert, J., and Phillips, W. "Information Processing in the Child: Significance of Analytic and Reflective Attitudes." *Psychological Monographs*, 1964, 78, No. 1 (whole No. 578).

Kagan, J. Personal communication, 1966.

Karp, Joan M. and Sigel, I. "Psychoeducational Appraisal of Disadvantaged Children." *Review of Educational Research*, 1965, 35 5, 401-412.

Loban, W. "Language Proficiency and School Learning." In J. D. Krumboltz (Ed.) *Learning and the Educational Process*. Chicago: Rand-McNally, 1965.

McNeil, W. *Rise of the West*. Chicago: University of Chicago Press, 1963.

Martin, C. J. Personal communication, 1965.

Milner, Esther. "A Study of the Relationship Between Reading Readiness in Grade One School Schildren and Patterns of Parent-Child Interaction." *Child Development*, 1951, 22, 95-112.

Murray, H. A. *Explorations in Personality*. New York: Oxford University Press, 1938.

Pace, C. R. and Stern, G. G. "An Approach to the Measurement of Psychological Characteristics of College Environments." *Journal of Educational Psychology*, 1958, 49, 269-277.

Shulman, L. S. and Keislar, E. R. (Eds.) *Learning by Discovery: A Critical Appraisal*. Chicago: Rand-McNally, in press.

Strodtbeck, F. "The Hidden Curriculum in the Middle-Class Home." In J. D. Krumboltz (Ed.) *op. cit.*

Wolf, R. W. "The Identification and Measurement of Environmental Process Variables Related to Intelligence." Unpublished Ph. D. Dissertation, University of Chicago, 1964.

Dr. J. Kenneth Morland:

RESPONSE TO DR. SHULMAN'S REMARKS

Professor Shulman's paper is a creative, thoughtful, and at the same time, provocative statement regarding the direction in which educational research should go. There are many ideas with which I find myself in agreement, and in my response I am tempted to add my support to them. However, I prefer to turn to parts of the paper about which I have some question and on which I would like clarification and discussion. This is in accord with what I feel that a conference of this sort has as a major purpose; namely, the clearing of ground and the giving of perspective.

Basic Versus Applied Research

The first of these questions concerns the role of the behavioral scientist in implementing social change in the area of education and thereby involves us with the title of the conference. Dr. Shulman does not deal with this question of role directly, but he does touch upon it indirectly toward the end of this paper when he states that in suggesting various types of research he is advocating not only applied investigations but basic research as well. He goes ahead to say that while the purpose of research in the behavioral sciences is the acquisition of knowledge about human behavior, there are many kinds of knowledge, including "knowledge of" and "knowledge for." However, after making this distinction, Dr. Shulman then proceeds to blur it -- at least for me -- by adding, "It is impossible to conduct truly basic research, yet select variables under investigation conditions that are amenable to change and hence can serve as input for educational programs and experimental study."

I'm not sure that I agree, although clarification of exactly what is meant would no doubt help.

I feel that a distinction needs to be maintained between the role of the behavioral scientist in carrying out basic research and his role in conducting applied research. My chief reason for taking this position is that I do not believe that theoretical and practical problems can be studied at the same time; furthermore, I do not feel that it is profitable even to try to do so. Basic and applied research have different goals and serve different functions. The goal of basic research is toward the refinement of theory. Of course the knowledge gained may be used to help with a practical problem, but this becomes application at a different time and in a different context. The goal of applied research is to solve a particular practical problem. It may draw upon knowledge and techniques from basic research -- indeed, if it does not, it would probably be ineffective -- and it may serve as a means of pragmatic testing of theory and of suggesting hypotheses for further study. In these ways applied research can make an indirect contribution to the building of theory. However, its primary aim is to solve a current, usually pressing problem.

There is another difference. The values that enter into the choice of doing theoretical research differ from those that enter into the choice of carrying out applied research. In the latter a value judgment about the desirability of a particular alteration in behavior is made, while in the former it is the value of the increasing of knowledge itself that is primary. These are two different kinds of judgment, in my opinion, and cast the behavioral scientist in two different roles. As has already been indicated, I think that both are important in implementing social change -- applied

research being directly related to such implementation and fundamental research, indirectly related. And I agree with Dr. Shulman that to get into a "status derby" in trying to determine which is more important is foolish and fruitless.

I believe furthermore that when behavioral scientists seek to implement social change, they are acting in their applied or practical role. This means they are making non-scientific value decisions to use their skills to help bring about a particular end they consider desirable in the society. I feel that such a value decision is made when we conduct educational research in order to bring about "optimal conditions for human growth." In fact, any goal of research other than the development of theory is not, in my opinion, basic or fundamental research. Now I warmly share this goal of educational research with Dr. Shulman, and most of the rest of what I have to say will talk about ways to obtain it. However, what I will be presenting later is applied research, directed toward the goal of developing the talent of all children.

Perhaps you will protest that I am trying to draw too sharp a line between the roles of social scientists in basic and applied research. But I feel that the distinction becomes especially important in the light of the disagreement among a number of behavioral scientists over the question of whether or not the behavioral scientist has any responsibility or obligation as a scientist to apply his research or use his skills in what he considers to be socially constructive endeavors. This issue continues to be a matter of lively debate.

The issue was discussed, for example, at the 1961 annual meeting of the Society for the Study of Social Problems at a session on the responsibilities of sociologists, and it was the chief issue in the presidential address

at that meeting by Alvin W. Gouldner of Washington University who spoke on "The Conception of a Value-Free Social Science as an Ideology." In addition, The Society for Applied Anthropology devoted most of a recent issue of *Human Organization* to presentations made at a symposium entitled, "Values in Action," which dealt with problems of the roles of anthropologists in applied fields. And the Society for the Psychological Study of Social Issues sponsored a symposium entitled, "The Roles of Social Science in Desegregation," the results of which were published in August of 1958. A recurring issue in these symposia is whether or not the social scientist has the obligation not only to contribute to his discipline as a scientists, but also whether through his discipline he should seek to implement values in his society. Thus, R. Nevitt Sanford of Stanford speaks of a major role of the social scientist to be that of a social reformer, but Conrad Arensberg of Columbia states that social scientists have no other end or purpose as scientists than devotion to science itself. Robert Bierstedt of New York University believes that normative judgments are outside the realm of science, while Robert Redfield, late of Chicago, states that value deductions are, in fact, drawn by social scientists from their science, and Alvin Gouldner believes that the concept of a value-free sociology is a myth.

I doubt that any consensus to this question can be reached. Each behavioral scientist must answer it for himself, and there will be differences in their answers. My own resolution of the matter -- as I've tried to do basic and applied work in the field of race relations -- has hinged in part on the insistence upon a distinction in roles. I find it hard to see how behavioral science as a discipline can either demand or forbid that its adherents apply their knowledge to practical problems or take part in directed change. This

is the sort of decision that takes us outside the realm of science. I feel that the behavioral scientist is carrying out the only job his discipline requires when he does basic research toward the end of developing sound theory. At the same time, the scientist has the option of working toward particular ends in terms of his values by applying his research or consultation skills without the restriction of that discipline.

I am frequently asked by colleagues -- as I am sure others of you in the behavioral sciences are -- "How can you conduct basic research in an area in which you have such clear-cut biases?" My answer is that these are two different kinds of jobs. When you accept the task as a consultant or you work for an organization to solve its particular problems, you are making the kind of value decision that is different from the one you make in conducting basic research....There seems to me to be two requests that those who are directly involved in bringing about social change can ask of behavioral science. (And I was somewhat concerned about Dr. King's remark last night: "At such a crucial time as this, then, the social scientist can render an invaluable contribution to our social order by being a catylyst, by becoming an activist, by stimulating, uplifting, reconciling democratic change." I am not so sure that this is what the activist should be asking of the social scientist.) But I think there are two things that can be asked. First, you can ask and expect from the discipline as a whole sound theory about human behavior; second, you can seek to enlist the skills of those behavioral scientists who share your cause to help to solve particular, practical problems.

The utility of basic research and sound theory in the implementing of social change can be seen in the testimony and decision of the Supreme Court in

1954. When those of us, invited by the NAACP to serve as expert witnesses, were asked on the stand whether or not legislation should actually change the way persons felt, we could cite the research of Samuel Stouffer, Morton Deutsch, Theodore Newcomb, and others to show that the most effective way to change the way people feel prejudice was to change the way people act. And in such witnesses as Dr. Kenneth Clark, the NAACP found persons who were willing to convey the knowledge and conduct the research that helped to convince the jurors. Perhaps what we need are liasion persons from both behavioral science and from activist groups.

Understanding the "Primary Environment"

...Turning next to some of the specific suggestions Dr. Shulman offers for the reconstruction of educational research, I find his emphasis on understanding of what he terms the "primary environment" especially intriguing. He reminds us that the effects of the primary environment are not sufficiently understood by most teachers. They tend to look upon their pupils as blank tablets upon which they can write anything, or as individuals who vary only in terms of supposedly immutable characteristics. I agree that we need to learn more of how this primary environment operates, and sociology and cultural anthropology can join with those in education to achieve this.

Dr. Shulman goes on to state that the greater the dissimilarity of the background between the primary environment and the classroom the greater the difficulty of the transfer of knowledge. I would state this with a different sort of emphasis....I'm not so sure that the homes from which even upper class children come are like a classroom. In fact, I think it is the school that makes the home adjust to fit in with homework and other assignments. I think

it is more to the point to state that some homes are ill equipped to give their children the kind of support and help they need when they enter school.

At least that was the case with white mill village children in a South Carolina town that I have studied on two separate occasions....Observation revealed that the background of the mill children did not make it likely they would succeed in school. Their parents could not help them with their studies and even had ambivalent feelings about schooling. There were few books in mill homes; no place to study. The language productivity of the children was low, and they tended to see their world in immediate, concrete terms.

I'm willing to use the term "culturally disadvantaged" to describe these children. I realize there are difficulties in using the term since it does not designate the degree or type of disadvantage. However, we can think of a continuum with greater and lesser degrees of being disadvantaged. In any event, it is incumbent upon us to examine the particular type of disadvantage. However, I'm concerned that some teachers continue to be unaware of disadvantaged primary environments and their consequences.

Self-Concept and Environment

Another aspect of the educationally disadvantaged that requires further study is the development of self-concept, and this is a matter that is related to the larger, societal environment, as well as to the primary environment. Among the social-psychological needs of all human beings, for mental and emotional well being, is the necessity to have others think well of us and the necessity to think well of ourselves. And the chief basis for thinking well of ourselves is what others think of us. Charles Horton Cooley many

years ago coined the expression "looking-glass self" as a means of demonstrating this. Cooley stated that others become the looking-glass in which we see ourselves. If we see ourselves as approved, we feel self-approval; if we see ourselves as disapproved, we have similar feelings for ourselves.

We do see ourselves as others see us. When we gave the Rorschach and other tests, they showed that the mill people looked down on themselves, taking over the attitude of the dominant group toward them. But at least they were white, and had the approval of the larger society. Negroes have seen themselves in the looking-glass of larger society as persons of inferior status, and this has affected their self-concept. Research among very young Negro children, pioneered so effectively by Kenneth and Mamie Clark, has shown consistently that these children prefer and identify with members of the dominant white race and therefore reject their own identity. Research shows that this happens before children can verbalize correctly about race differences, demonstrating that such self-conception is the product of "absorption" from the environment rather than the result of any direct or specific teaching. We have sufficient research, particularly that of Wilbur Brookover and Richard Morse, to show that self-concept of ability is directly related to academic achievement -- the lower the self-concept, the lower the achievement; the higher the self-concept, the higher the achievement.

These are among some of the knowledge that we have, of which Dr. Hauser was speaking last night, and that we are not putting to effective use. A next step in research in this area is to find out how to help teachers learn to accept and to believe in the culturally disadvantaged. Few things are more important to the success of the child in school than is the attitude of the teacher toward him.

Effects of Standardized Testing

I would like for us to take a closer look at the effects of standardized testing. I agree with Dr. Shulman that schools as they are now set up often, but certainly not always, perpetuate social distinctions and preserve the status quo. I feel that one way this is done is through the program of standardized testing which acts as a factor selecting the more advantaged children for special attention. The Russell Sage Foundation is developing a series of studies that could be read and pursued with profit by all concerned with giving genuine equality of opportunity to American children. The first volume, *The Search for Ability*, by David Goslin, cites evidence to show that such testing provides a self-fulfilling prophecy. Those the tests select as best are then given special attention and treatment and are, in effect, told by their looking glass that they are especially able. This is not to question that children have differences in capacity and in ability. But the question is, how closely related is the testing to the actual experience of children so it reveals the ability of children to learn? The mill children, for example, were bright and quick on possum hunts and in joking relations with their kinfolk....But the kinds of testing to which they were subjected to at school revealed little of the ability required to learn these out-of-school skills. Can tests of what is learned in coping with actual surroundings be made in order to reveal more clearly the ability to learn? We can then avoid the mistake, as Robert Green points out, that comes with the confusion of the educationally retarded with the intellectually retarded.

Attitudes About Culture and Race

A final suggestion concerning educational research and practice has to do with discovering what ideas about culture and race are held by teachers and school

administrators, and what transmission about these concepts are made to pupils. I continue to be concerned about the highly damaging effects of racism in the educational system. Perhaps I am particularly sensitive to this issue because of where I live, in a town near Henry Garrett, and in an office near Audrey Shuey. But I could also see the effects of racist ideology and the belief that race is paramount in the development of culture on my assignments with the Community Relations Service, particularly in Selma. What kinds of teaching about race and culture can be most effective with teachers and their pupils? I realize the difficulty of introducing scientific notions about race and culture in some areas because of social and political attitudes. However, behavioral scientists should begin now to develop materials and methods for such teaching.

Let me add to Dr. Shulman's strategies for research one additional suggestion that I made last month for Duke's Center for Southern Studies. This involves the cooperative efforts of a inter-disciplinary team in the intensive study of cultural transmission in selected communities. Such studies would give the opportunity to observe and compare behavior in its full and natural setting, or "in vivo" as Conrad Arensberg has phrased it. This would be in contrast to studies "in vitro" which depend upon abstraction and statistical comparison. I think that studies "in vitro" are continuously needed, but also needed are those studies done in context. This would offer an excellent opportunity for determining the effects of the primary environment and for checking how well standardized tests reflect the actual learning ability of children. I feel that cultural anthropologists and sociologists could profit from the cooperation and coordination of their efforts with psychologists and those in the field of education especially. Furthermore, this would give an

opportunity to carry out those epidemiological studies suggested by Shulman by comparing in the same environment children of varying achievement levels.

I think there is no more important problem for education and for the nation than to develop the full potential of the disadvantaged in our society. The President stated this forcefully in a talk at Howard University about a year ago when he said it is not enough to open the door to opportunity, we must be sure that all of our children have the chance to walk through it. The President went on to say that you cannot handicap people for generations and then expect them all at once to throw off those handicaps and compete equally with the more privileged. I think that the reconstruction of educational research that Dr. Shulman suggests can help to make that competition more equal and thereby help to create optimal conditions for human growth.

Dr. Herman H. Long:

RESPONSE TO DR. SHULMAN'S REMARKS

It is risky to re-state the conclusions and ideas of an author, and especially when the argument has been tightly reasoned and based upon careful sifting of the pertinent bodies of knowledge, as in the case of Dr. Shulman's excellent paper. Distortion is likely to occur -- and perhaps misrepresentation as well, but the damage -- if there be any -- can be repaired as long as the author himself is present to provide the necessary correctives.

I believe that Dr. Shulman has done this conference and the larger educational community a real service in preparing this paper, for he has thrown widely open the door of inquiry into a major direction of educational practice of importance to social change and which only a few suspecting educators have peered through with expressed concern. I shall, undoubtedly, over-generalize his ideas, but I hope that my doing so will serve the useful purpose of sharpening the issues which are both explicit and implicit in Dr. Shulman's paper.

I deduce from Dr. Shulman's presentation the conclusion that educational practice, including the research which has either given origin to such practice or derived from it, has diverted from its original early-twentieth century revolutionary course, provided by the truly great minds of American psychologist-educators -- William James, John Dewey, E. L. Thorndike and others -- toward a set of preoccupations which seriously limit its serviceability to both human and social needs in these days of complex and accelerating change.

The major distortions which I discern are of two general and related categories: a dominant preoccupation in educational strategy and attack with measurement of the so-called mental abilities and with the extent of learning, on the one hand, and consuming interest, on the other, with what Dr. Shulman has called the insistence upon behavioral definitions of objectives for instruction. And I gather that he means by this, specific, discrete and limited objectives as in the case of response measurements of test items, combinations of items and even test batteries. The effect of this has been to allow instrumentation to supplant broad educational objectives as the controlling factor in the educational process, a case not only of the tail wagging the dog but also of the substitution of means for ends.

What eventuates is a situation in which the classification of human beings in terms of ability, performance and potential has become a...dominant theme of the educational process, and to the extent that human variability is taken into account it is, as Shulman expresses it, largely in terms of "individual differences in supposedly immutable characteristics." And to the degree that educational programming and the instructional processes are based upon this major departure, one can easily agree that it is a situation in which human adaptability to the vicissitudes of social expectation is not only frustrated but, also, one in which the school primarily succeeds in maintaining the status quo.

While I do not agree with those aspects of Dr. Shulman's analysis which treat laboratory experimental psychology as a sterile source of educational theory and a substantial cause for the present state of affairs in the educational establishment, I concur with his prescription: making the central

focus of research and instruction the *processes* through which changes in human responsiveness and motivation take place; making prior experience relevant to formal school learning through meaningful knowledge of the nature of the pre-conditioning environments, adopting longer-range longitudinal strategies for the determination of educational objectives and programming; discarding both methodological and classificatory preconceptions and allowing problems to dictate strategy; and concentrating upon an engineering approach which, instead of beginning with theory and principles, works back from the successes and partial successes of rigorously designed programs to an analysis of the probable determining factors.

Whether current educational practice can, in effect, reverse itself and meet such demands is another matter. The strange and gnawing anomaly of our day is that in the face of the most fundamental revolution in the status and expectations of groups historically excluded from the opportunity in the common society, as well as the economic, political and technological pressures which are greatly enhancing and unprecedented and broadscale social mobility for everyone, the vehicle best equipped to feed and sustain these processes -- the schools and institutionalized education -- seems to be tragically floundering in its own contrivances. It appears to me to have neither the direction of attack or philosophical ground commensurate with the demands of social revolution and new human expectations. The best departure which it has yet been able to launch, to match these new demands, is a doctrine of compensatory intellectual repairment, as best seen in the now widely, dogmatized versions of so-called cultural deprivation. The difficulty I see here is that a concept of useful functional significance to the learning process has taken on the

characteristics of an entity - and even more an entity of absolute and categorical dimension -- at least in its programmatic application by the educational establishment. What we have in this case is a new kind of quantity developed into a new educational formula, not unlike the limiting educational categorizations of I.Q. and racially-determined abilities. Indeed, I would argue that in its unduly popularized applications cultural deprivation has become a new, though benign, expression of the doctrine of racial inferiority.

Thus, as Dr. Shulman might express it, "educational rhetoric" reduces a potentially useful body of psychological insight to a form of education fadism. I see in this matter some rather deep and sensitive issues of social and educational policy of which we discern at present only a faint but disturbing glimmer.

Witness, for example, two very recent phenomena: the strike of Negro students at Northern High School in Detroit against a school administration and teachers whom they accuse of placing lowered standards of intellectual demand upon them because of assumptions of categorical cultural deprivation, and the case, in the second instance, of a liberal candidate for governor in Tennessee campaigning on a platform of positive support of school integration but with assurances to establish a uniform public school program of ungraded instruction.

The latter strikes me as a stop gap tactic, adopted certainly from the lexicon of enlightened educators, but used primarily to assuage the anxieties of both white parents and teachers still bound to the assumptions and expectations of a racial ideology. The issue, not yet joined in these terms exists on a wide front, whether it be the unmet challenge of *de facto* segregation,

the legislative omission of positive integration, the conflicting court decisions on racial balance, the legitimacy or illegitimacy of bussing for purposes of integration, or the selective integration of Negro students and teachers (largely middle and upper class) as over-against the application of uniform policy.

I suggest that we will need to look, always alertly and critically, at the assumptions which underlie educational programming for the revolution now at work. And whether it is a research and evaluative effort or an instructional innovation, we will need to see both the immediate and long-range implications for both public and educational policy. Otherwise our best efforts to engineer social and educational reform will be caught in unintended entrapment of basic policy conflicts. The lure of the expedient and popular vehicle is perhaps the greatest danger which we face in this respect.